raised into position. The pipette with the crook is then taken out of the fluid, and inverted. The crook is laid aside, and the outside of the pipette cleaned with blotting-paper, by which all adhering foreign grains are removed. The pipette now contains a pure gathering of the mineral required, and it only remains to discharge its contents, and this is of course accomplished by pushing down the piston.

Minerals may thus be removed from every zone of a diffusion column; and all the species which enter into the composition of a rock, except of course the very heaviest, may be separately obtained, with their specific gravity determined as an incident of the process.

In this absurdly simple fashion "getting it out" ceases to be a penance, and becomes a pleasure. W. J. SOLLAS.

THE CLOUDY CONDENSATION OF STEAM.[1]

THE air, as every one knows, is composed almost entirely of the two gases, oxygen and nitrogen. It also contains small quantities of other substances, of which the chief are carbonic acid gas and water vapour, and it is the latter of these constituents, water vapour, or "steam," as it is sometimes called, that will principally concern us this evening.

The quantity of invisible water vapour which the air can at any time take up depends upon the temperature; the higher the temperature of the air the more water it can contain. The proportion, however, never exceeds a few grains' weight of water to a cubic foot of air. Air at any temperature, containing as much water as it can possibly hold, is said to be "saturated," while the temperature at which air containing a certain proportion of water becomes saturated is called the "dew point."

The large glass globe, upon which the beam from the electric lantern is now directed, contains ordinary air, kept in a state of saturation, or nearly so, by the presence of a little water. You will observe that although heavily laden with water vapour the air is perfectly transparent. If, now, we turn a tap, and so connect the globe with an exhausted receiver, the air expands and becomes colder; the space inside the globe is no longer able to hold the same quantity of water as before in the form of vapour, and the excess is precipitated as very finely divided liquid water, which fills the globe and is perfectly visible as a cloud or mist. In a few minutes the cloud disappears, partly, no doubt, because some of the particles of water have fallen to the bottom of the vessel, but chiefly because the air becomes in time warmed up to its original temperature (that of the room), and the suspended water is converted back again into invisible vapour.

I once more rarefy the air, and admit a fresh supply while holding the flame of a spirit lamp near the orifice of the inlet pipe, so that some of the burnt air is carried into the interior of the globe. When the air is again expanded a cloud is formed which is far more dense than the others were. It appears on examination that the increased density of this cloud is not due to the condensation of a greater quantity of water. Little, if any, more water is precipitated than before. But the water particles are now much more numerous, their increased number being compensated for by diminished size. Within certain limits, the greater the number of particles into which a given quantity of water is condensed, the greater will be the apparent thickness of the mist produced. A few large drops will not impede and scatter light to the same extent as a great number of small ones, though the actual quantity of condensed water may be the same in each case.

Then comes the question, why should the burnt air from the flame so greatly increase the number of the condensed drops? An answer, though perhaps not quite a complete one, is furnished by some remarkable experiments made by M. Coulier, a French professor, nearly twenty years ago. He believed his experiments showed that water vapour would not condense at all, even at temperatures far below the dew point, unless there were present in the air a number of material particles to serve as nuclei around which the condensation would take place. All air, he says, contains dust; and anything that increases the number of dust particles in the air increases the density of the condensation by affording a greater number of nuclei. Air in which a flame had been burnt he supposed to be very highly charged with finely-divided matter, the products of combustion, and thus rendered extraordinarily "active" in bringing about condensation. And that, according to Coulier's view, is the reason why such a dense fog was formed when air which had been contaminated by the spirit flame was admitted to our globe.

On the other hand, air, even burnt air, which has been filtered through tightly packed cotton wool, is found to be perfectly inactive. No cloud or mist will form in it, however highly it may be supersaturated. Coulier explained this fact by supposing that the process of filtration completely removed all dust particles from the air.

The experiments of Coulier were repeated and confirmed by Mascart. The latter also made one additional observation which may very probably turn out to be of great importance. He found that ozone, or rather, strongly ozonised air, was a very active mist producer, and that unlike ordinary air, it was not deprived of its activity by filtration.

Four or five years later, all the facts which had been noticed by Coulier, and others of an allied nature, were independently discovered by Mr. Aitken, who has devoted much time and study to them, and made them a foundation of an entirely new branch of meteorology.

Later, perhaps, we may see reason to doubt whether all the conclusions of Coulier and Aitken are quite accurate, especially as regards the action of so-called products of combustion.

Every one has noticed how dense and dark a thundercloud is. It shuts out daylight almost as if it were a solid substance, and the glimmer that penetrates it is often imbued with a lurid or copper-coloured tint.

I had always found it rather difficult to believe that these peculiarities were due simply to the unusual extent and thickness of the clouds, as is commonly supposed to be the case, and it occurred to me about three years ago, that perhaps some clue to the explanation might be afforded by the electrification of a jet of steam. On making the experiment I found that the density and opacity of the jet were greatly increased when an electrical discharge was directed upon it, while its shadow, if cast upon a white screen by a sufficiently strong light, was of a decidedly reddish-brown tint.

As a possible explanation of the effect I suggested that there might occur some action among the little particles of water of a similar nature to that observed by Lord Rayleigh in his experiments upon water jets. A jet of water two or three feet long is made to issue in a nearly vertical direction from a small nozzle. At a certain distance above the nozzle the continuous stream is found to break up into separate drops, which collide with one another, and again rebounding, become scattered over a considerable space. But when the jet is exposed to the influence of an electrified substance, such as a rubbed stick of sealing-wax, the drops no longer rebound after collision, but coalesce, and the entire stream of water, both ascending and descending, becomes nearly continuous.

There is one other point to which I wish to direct your particular attention. If the sealing-wax, or better, the knob of a charged Leyden jar, is held very close to the jet, so that the electrical influence is stronger, the separate drops do not coalesce as before, but become scattered even more widely than when no electrical influence was operating. They become similarly electrified and, in accordance with the well-known law, repel one another.

We will now remove the water jet, and in its place put a little apparatus for producing a jet of steam. It consists of a half-pint tin bottle, through the cork of which passes a glass tube terminating in a nozzle. When the water in the bottle is made to boil, a jet of steam issues from the nozzle, and if we observe the shadow of the steam jet upon the screen we shall see that it is of feeble intensity and of a neutral tint, unaccompanied by any trace of decided colour. A bundle of needles connected by a wire with the electrical machine is placed near the base of the jet, and when the machine is worked electricity is discharged into the steam. A very striking effect instantly follows. The cloud of condensed steam is rendered dense and dark, its shadow at the same time assuming the suggestive yellowish-brown colour.

I at first believed that we had here a repetition, upon a smaller scale, of the phenomenon which occurs in the water jet. The little particles of condensed water must frequently come into collision with one another, and it seemed natural to suppose that, like Lord Rayleigh's larger particles, they rebounded under ordinary circumstances, and coalesced when under the influence of electricity. The great majority of the small particles ordinarily formed consisted, I thought, of perhaps only a few molecules, which were dispersed in the air, and again converted into vapour without ever having become visible, while the larger particles formed by their coalescence under electrical action were of such dimensions as to impede the more refrangible waves of light. Hence the brownish-yellow colour.

---

[1] Extracted from a lecture on "Fogs, Clouds, and Lightning," delivered at the Royal Institution on May 5.

Other explanations have been proposed. There is the molecular shock theory of the late R. Helmholtz (who, as it turned out, had studied electrified steam jets before I made my own experiments). I shall refer to his speculation later. And there is the dust-nucleus theory, which no doubt appears a very obvious one.

Though I knew that my own hypothesis was not quite free from objection, neither of these alternative ones commended itself to me as preferable; and so the matter rested until a few months ago, when the steam jet phenomenon was discussed anew in a paper communicated to the Royal Society by Mr. Aitken. Mr. Aitken said that he did not agree with my conjecture as to the nature of the effect. This led me to investigate the matter again, and to make some further experiments, the results of which have convinced me that I was clearly in error. At the same time it seems to me that the explanation which Mr. Aitken puts forward is little less controvertible than my own. Mr. Aitken's explanation of the phenomenon is, like mine, based upon Lord Rayleigh's work in connection with water-jets, but, unlike mine, it depends upon the experiment which shows that water particles when strongly electrified are scattered even more widely than when unelectrified. He believes, in short, that electrification produces the effect, not by promoting coalescence of small water particles, but by preventing such coalescence as would naturally occur in the absence of electrical influence. In the electrified jet, he says, the water particles are smaller, but at the same time more numerous; thus its apparent density is increased.

The chief flaw in my hypothesis lies in the fact that the mere presence of an electrified body like a rubbed stick of sealing-wax, which is quite sufficient to cause coalescence of the drops in the water jet, has no action whatever upon the condensation of the steam jet. There must be an actual *discharge* of electricity. But it is by no means essential, as Mr. Aitken assumes, that this discharge should be of such a nature as to electrify, positively or negatively, the particles of water in the jet. If, instead of using a single electrode, we employ two, one positive and the other negative, and let them spark into each other across the jet, dense condensation at once occurs. So it does if the two discharging points are removed quite outside the jet. A small induction coil giving sparks an eighth of an inch in length causes dense condensation when the electrodes are more than an inch distant from the nozzle and on the same level. In one experiment a brass tube two feet long was fixed in an inclined position with its upper end near the steam jet, and its lower end above the electrodes of the induction coil. In about three seconds after the spark was started dense condensation ensued, and it ceased about three seconds after the sparking was stopped. No test was needed, though in point of fact one was made, to show that the steam was not electrified to a potential of a single volt by this operation. And the time required for the influence to take effect showed that whatever this influence might be it was not induction.

The inference clearly is that in some way or other the action is brought about by the air in which an electrical discharge has taken place, and not directly by the electricity itself. The idea has no doubt already occurred to many of you that it is a dust effect. Minute particles of matter may be torn off the electrodes by the discharge, and form nuclei upon which the steam may condense. The experiments of Liveing and Dewar have indeed shown that small particles are certainly thrown off by electrical discharge, and the idea that such particles promote condensation appears to be supported by the fact that if a piece of burning material, such as touch-paper, is held near the jet so that the products of combustion can pass into it, thick condensation is produced.

From a recent paper by Prof. Barus, published in the *American Meteorological Journal* for March, it appears that he also is of opinion that such condensation is in all cases due to the action of minute dust particles. Yet it is remarkable that Mr. Aitken, the high priest and chief apostle of the philosophy of dust, gives no countenance to the nucleus theory. He does not even advert to its possibility. I imagine that his experiments have led him, as mine have led me, to the conclusion that it is untenable. And this not only in the case of electrical discharge, but also in the case of burning matter.

If we cause an electrical discharge to take place for some minutes inside a suitably arranged glass bottle, and then, ten or fifteen seconds after the discharge has ceased, blow the air from the bottle into the steam jet, the condensation is not in any way affected. Yet the dust could not have subsided in that time. And again, if we fill another large bottle with dense clouds of smoke by holding a bundle of burning touch-paper inside it, and almost immediately after the touch-paper is withdrawn, force out the smoke-laden air, through a nozzle, upon the jet—you can all see the black shadow of the smoke upon the screen—nothing whatever happens to the jet. Yet a mere scrap of the paper which is actually burning, though the ignited portion may not be larger than a pin's head, at once darkens the jet. Dead smoke (if I may use the term) exerts little or no influence by itself: there must be incandescent matter behind it. The question naturally arises, whether incandescent matter may not be sufficient of itself, without any smoke at all. We can test this by making a piece of platinum wire red-hot and then holding it near the jet. It is seen to be quite as effective as the burning touch-paper. Yet here there can be no nuclei formed of products of combustion, for there is no combustion; there is simply ignition or incandescence.

One other point I may mention. It is stated by Barus, in the paper above referred to, that the fumes given off by a piece of phosphorus constitute a most efficient cause of dense condensation. This is true if they come directly from a piece of phosphorus; but if phosphorus fumes are collected in a bottle and then directed upon the jet, all traces of unoxidised phosphorus being first carefully removed, they are found to be absolutely inoperative. Phosphorus in air can hardly be said to be incandescent, though it is luminous in the dark; but it appears to act in the same manner as if its temperature were high.

All these facts seem to indicate that the several causes mentioned, electrical, chemical, and thermal, confer upon the air in which they act some temporary property—certainly not due to mere inert dust—in virtue of which it acquires an abnormal power of promoting aqueous condensation.

I thought that possibly some clue as to the nature of this property might be obtained by observing how some other gases and vapours behaved; but though the experiments I made perhaps tend to narrow the dimensions of the mystery, I cannot say that they have completely solved it. Indeed some of the results only introduce additional perplexities.

One of the most natural things to try is hydrochloric acid, which is known to have a strong affinity for water. If we heat a little of the acid solution in a test tube, closed with a cork, through which a glass tube is passed, and direct the issuing stream of gas upon the jet, the densest condensation results. The vapours of sulphuric and nitric acids also cause dense condensation, and I suppose both of these have an affinity for water. But so also, and in an equally powerful degree, does the vapour of acetic acid; yet the affinity of this acid for water, as indicated by the heat evolved when the two are mixed, is very small.

Ammonia gas, when dissolved in water, causes the evolution of much heat. Yet a stream of this gas directed upon the jet has no action.

Ozonised air, which Mascart found so effective in his experiments with the closed vessel, is quite inoperative with the steam jet. Equally so is the vapour of boiling formic acid, which I believe is chemically a much more active acid than acetic, and has a lower electrical resistance. (See Table.)

CONDENSATION OF STEAM JET.

Active.

Air, oxygen, or nitrogen, in which electrical discharge is occurring.

Burning and incandescent substances.
Fumes from phosphorus.
Hydrochloric acid.
Sulphuric acid vapour.
Nitric acid vapour.
Acetic acid vapour.

Inactive.

Air, &c., in which electrical discharge has ceased for about ten seconds.

Smoke without fire.
Bottled phosphorus fumes.
Ammonia.
Ozone.
Steam.
Alcohol vapour.
Formic acid vapour.
Sulphurous acid.

It seems that we have here a pretty little problem which might, perhaps, be solved without much difficulty by a competent chemist, but which quite baffles me.[1] Is it possible that the condensing vapours may contain dissociated atoms?

To return to the electrical effect. There are only two kinds of chemical change that I know of which could be brought about in air by an electrical discharge. Either some of the oxygen might be converted into ozone, or the oxygen and nitrogen of the air might be caused to combine, forming nitric acid or some such compound. The former of these would not account for the action of the air upon the jet, because, as we have seen, ozone is inoperative; the latter might. But if the activity of the air is due to the presence in it of a compound of oxygen and nitrogen, then it is clear that an electrical discharge in either nitrogen or oxygen separately would fail to render those gases active.

I arranged a spark bottle, inside which an induction-coil discharge could be made to take place; two bent tubes were passed through the cork, one reaching nearly to the bottom for the ingress of the gas to be tested, the other, a shorter one, for its egress. The open end of the egress tube was fixed near the steam jet, and first common air, then oxygen and then nitrogen were successively forced through the bottle while the coil discharge was going on. All produced dense condensation, but I thought that oxygen appeared to be a little more efficient than common air, and nitrogen a little less.

This last experiment points to a conclusion to which at present I see no alternative. It is that the action on the jet of an electrical discharge is due in some way or other to dissociated atoms of oxygen and nitrogen. There is nothing else left to which it *can* be due.

So far as Robert Holmholtz's explanation coincides with this conclusion, I think it must be accepted as correct. As to the precise manner in which he supposed the dissociated atoms to act upon the jet, it is more difficult to agree with him. He thought that the abnormal condensation was a consequence of the molecular shock caused by the violent recombination of the dissociated atoms in the supersaturated air of the jet, the action being analogous to that which occurs when a supersaturated solution of sulphate of soda, for example, is instantly crystallised by a mechanical shock.

To me this hypothesis, ingenious as it is, seems to be more fanciful than probable, but I can only hint very diffidently at an alternative one. To many chemical processes the presence of water is favourable or even essential. Is it possible that the recombination of free atoms may be assisted by water? And is it possible that dissociated atoms in an atmosphere of aqueous vapour may obtain the water needed for their union by condensing it from the vapour?

According to Holmholtz, flames and incandescent substances generally cause dissociation of the molecules of oxygen and nitrogen in the surrounding air. This, I believe, is generally admitted. I do not know whether slowly oxidising phosphorus has the same effect.

If it is conceded that the atmospheric gases are dissociated by electrical discharges, and that the presence of such dissociated gases somehow brings about the dense condensation of water vapour, we may still regard the electrified steam jet as affording an illustration of the abnormal darkness of thunder-clouds.

Perhaps another source of dissociated atoms is to be found in the ozone which is generated by lightning flashes. A molecule of ozone consists of three atoms of atomic oxygen, while one of ordinary oxygen contains only two. Ozone is an unstable kind of material, and gradually relapses into ordinary oxygen, the process being that one atom is dropped from the three-atom molecules of ozone, these detached atoms in course of time uniting with one another to form pairs. Thus two molecules of ozone are transformed into three of oxygen. A body of ozone is therefore always attended by a number of dissociated atoms which are looking for partners.

In the steam jet experiment there is not time for the disengagement of a sufficient number of isolated atoms from a blast of ozone to produce any sensible effect. But the case is otherwise when the vapour is confined in a closed vessel, as in Mascart's experiment, or when it occurs in the clouds, where the movement of air and vapour is comparatively slow.

Ozone, it will be remembered, was found by Mascart to produce dense condensation in a closed vessel even after being filtered through cotton wool. Similar filtration seems to entirely deprive the so-called products of combustion of their active property, a fact which has been adduced as affording overwhelming evidence in favour of the dust nucleus theory. Coulier himself, however, detected a weak point in this argument. He produced a flame which could not possibly have contained any products of combustion except steam, by burning pure filtered hydrogen in filtered air; yet this product was found to be perfectly capable of causing dense condensation, and, as in his former experiments, filtration through cotton wool deprived it of its activity.

These anomalies may, I think, be to a great extent cleared up if we assume that the effect of the cotton wool depends, not upon the mere mechanical obstruction it offers to the passage of particles of matter, but upon the moisture which it certainly contains, and which may act by attracting and facilitating the reunion of dissociated atoms before they reach the air inside the vessel. According to this view ozone would remain an active condenser in spite of its filtration, because free atoms would continue to be given off by it after it had passed the cotton wool. The filtration experiment should be tried with perfectly dry cotton wool, which, however, will not be easily procured, and if my suggestion is right, dry wool will be found not to deprive ordinary products of combustion of their condensing power.

To sum up. I think my recent experiments show conclusively that the dense condensation of the steam jet is not due directly either to electrical action or to dust nuclei. The immediate cause is probably to be found in dissociated atoms of atmospheric gases, though as to how these act we can only form a vague guess.

SHELFORD BIDWELL.

SCIENTIFIC SERIALS.

American Journal of Science, December.—An apparent time-break between the eocene and Chattahoochee miocene in south-western Georgia, by Raphael Pumpelly. The Red Clay Hill region, a plateau extending through the south-western part of Georgia and adjacent northern Florida, has a maximum altitude of 300 feet, is sharply limited on the north by a declivity facing the eocene flat-land country, and consists of miocence deposits resting on eocene, both of which dip about 13 feet per mile to the south. The base of the plateau is formed by the white calcareous beds of the Chattahoochee group. A time-break between the latter and the eocene is evidenced by the almost general presence of a limestone conglomerate at the base of the Chattahoochee, immediately overlying eocene fossils, and the irregularity of the surface of demarcation. It seems possible that during miocene time the present plateau of southern Georgia was outlined by submerged islands of the eocene limestone. The Gulf Stream, after the creation of the central American barrier, found its way back to the Atlantic sweeping over southern Georgia and northern Florida, and supplying the food needed to build up the great organic beds of the Chattahoochee and Chipola. The lower flat-land country of central Georgia may represent the contemporaneous course of the cold current carrying less pure water and less nutriment.—The rise of the mammalia in North America, by H. F. Osborn. This second part deals with ancient and modern placental differentiation, the succession of the perissodactyls and the artiodactyls, a discussion of the factors of evolution, and a diagram illustrating the supposed descent of the mammalia from their jurassic prototypes.—On the thoracic legs of Triarthrus, by C. E. Beecher. Some very perfect specimens of *Triarthrus Becki*, Green, in which nearly the entire calcareous and chitinous portions are represented by a thin film of iron pyrites, show, besides the antennæ already noticed, a complete series of thor-

---

[1] Two chemists of the highest eminence have been good enough to consider the problem for me, but they are unable to throw any light upon it.